

being required for its measurement. The radium present in this case was about 1/100 that contained in the sample nine years old.

It was not to be supposed that an insufficient degree of purity was the cause of the large amount of radium in the old sample, nor is it probable that thorium itself slowly decomposes into radium. I therefore considered that in the technical preparation of thorium an active substance is separated with the latter, and in turn decomposes into radium; it is probably the direct parent substance of radium, for which search has been recently made. In order to test this view, an attempt was made to show the increase in the amount of radium in a solution of thorium; 100 grams of freshly prepared thorium nitrate was examined, the quantity of radium being ascertained. The amount of emanation collected after four days was used for calculating the equilibrium amount, which is reached after about a month. On August 17 the flask was sealed, and again tested on October 10; the amount was double as great as in the previous test.

Forty grams of thorium nitrate prepared at the end of April was examined in the same way and left during the vacation. In this case also there was a marked increase in the amount of radium.

Fifty grams of thorium nitrate made in June, 1907, was precipitated from acid solution with oxalic acid. In the filtrate the quantity of radium was determined, the same being also done in October. In each case the same result was obtained. The parent substance of radium had therefore been precipitated with the thorium. By mistake, the precipitated material was mixed with other thorium preparations, and could not, therefore, be further investigated.

About 1 mg. of radio-thorium (activity about 100,000) was freed as far as possible from radium (the small amount retained being determined) and sealed up on August 15. The solution on October 14 gave the same amount of radium emanation as before. The radio-thorium was prepared from thorianite by means of barium sulphate, and should not, therefore, have contained the parent substance of radium, as, indeed, was actually found to be the case.

Knowing the proportion of uranium and thorium in monazite sand, and assuming that all the thorium and the whole of the parent substance of radium are separated during the extraction of thorium, the life of radium can be calculated by determining the quantity of radium in a given weight of thorium of known age. I have assumed the monazite to contain on the average 0.3 per cent. of uranium and about 5 per cent. of thorium oxide. From this it follows that 1 gram of thorium nitrate in equilibrium with radium contains about 2.10^{-8} gram of radium bromide. From the values I have found with samples of thorium of different age, the period of decay for radium lies between 2000 and 3000 years. Alterations in the proportions of uranium and thorium would naturally cause corresponding deviations in the value of the constant. The values given, therefore, only indicate approximately the order of magnitude of the period of decay, since I am ignorant of the exact composition of the monazite used as a source of the nitrates investigated.

I hope to publish more accurate details shortly in another place.

OTTO HAHN.

Chemical Institute, Berlin.

The Victoria Jubilee Technical Institute, Bombay.

I SEE in NATURE of November 7 advertisements for a principal and professor of chemistry for the Victoria Technical College in Bombay. Though I have no connection with the institute, and may be charged with unwarrantable interference, I think that it is only fair to intending competitors that certain facts should be made known.

I wish to point out, in the first place, that the management of this institution is in the hands of a board of trustees, and that the principal is not a member of this board, nor has he the right of communicating with the board except through the medium of the honorary secretary. The title of principal does not even secure to the holder of it the exclusive right of calling meetings of the staff, and, in fact, confers nothing more than the power

to enforce discipline. On the occasion of the opening of the new laboratories in February last, I was much impressed by the fact that not only was the principal not among the speakers, but that he and his staff were barely referred to, and that their names only appeared in the descriptive pamphlet which was published for the occasion inside the cover and at the end.

Such were the conditions under which my friend Dr. Mackenzie held the appointment, and it was with no astonishment that I heard on my return from India that he had sent in his resignation. I may add that though Rs. 1000 per mensem with a residence appears to be a good salary, it must be remembered that there is no security of tenure of the appointment, and that the residence offered to Dr. Mackenzie lay between the dustiest road and the busiest railway in the heart of Bombay.

Should any chemist contemplate applying for the "chair" of chemistry with the view of carrying out research in his spare time and ultimately improving his position, I should like to remind him that he will do well to take his library with him. There are no scientific books in Bombay.

Before leaving Bombay I made it clear to some of my friends who are interested in the institute that, in the event of Dr. Mackenzie's resignation being accepted, I should make the facts public, and should warn other scientific men against accepting the appointment upon similar terms.

MORRIS W. TRAVERS.

London, November 9.

November Meteors.

THOUGH the general conditions under which the Leonid meteor shower of 1907 takes place are not the most favourable, still a display of moderate intensity may be expected. The shower promises to be most conspicuous on the night of November 16, when moonlight will interfere considerably with observations, especially in the case of the smaller meteors. The following are the times of the various maxima as computed by the writer, the results of these calculations being expressed in Greenwich mean time:—

Leonid epoch, November 15, 9h. The shower, which is of the third order of magnitude, succeeds the epoch, the principal maxima occurring on November 16, 17h., 17h. 30m., and 18h. 30m. There is also a weak secondary epoch on November 17, 10h., the shower in this case preceding the epoch, and having its principal maxima on November 16, 13h. 40m., 18h., and November 17, 2h.

The intensity of the maxima of a meteoric epoch is inversely as the order of magnitude of the shower connected with it. Two showers, though of different intensities, will, as may be seen, take place on the night of November 16.

Scattered through the rest of the month are several interesting minor showers, details of the most remarkable of which will now be given:—

Epoch, November 22, 4h. Shower of tenth order of magnitude. The shower precedes the epoch, the principal maxima occurring on November 20, 8h., November 21, 14h., and November 22, 3h.

Epoch, November 25, 12h. This shower, which is of the ninth order of magnitude, has its principal maxima after the epoch as follows:—November 26, 1h. 50m., November 27, 2h. 30m. and 6h. Of these, the latter two are the heaviest maxima.

Epoch, November 29, 18h. The shower, which is of the fifteenth order of magnitude, follows the epoch, the principal maxima occurring on November 29, 23h., November 30, 18h., and December 1, 4h.

Closely associated with the last shower is another, which occurs early in December, is of the fifth order of magnitude, and has its maxima on December 2, 11h., and December 3, 8h.

JOHN R. HENRY.

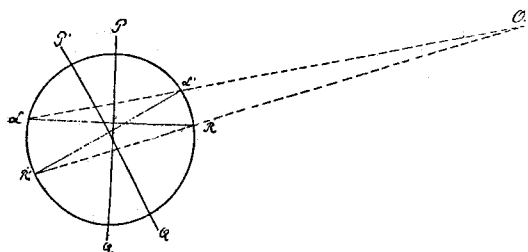
An Optical Illusion.

THE optical illusion described by Mr. Douglas Carnegie in NATURE of October 18, 1906, may be explained as follows:—

The thaumatrope generally reverses its apparent direction of rotation when the observer shuts one eye, or, better still, both eyes alternately, according to the physiological

law that the observation of a motion which is being stopped is in favourable circumstances followed by the perception of that motion in reversed direction.

The accompanying figure shows the card with its plane passing through the observer *O*. *L* and *R* are the intersections of the circumference of the card with an arbitrary horizontal line. Suppose that the card turns round its axis *PQ* from north to west, the observer will see *L* removing to the left and *R* to the right, and from this he can conclude the exact direction of the rotation, provided that he makes sure of *L* being behind and *R* before *PQ*, and not otherwise. For suppose *L* before *PQ* in *L'* and *R* behind *PQ* in *R'*, the observed removal of these



points respectively to the left and to the right would lead to an apparent opposite direction of rotation—from north to east. Moreover, the axis *PQ* perpendicular to *LR* would seem to tilt away from the vertical to *P'Q'* perpendicular to *L'R'*. Hence the illusion depends upon the following condition:—if *L* and *R* are seen in the right place with respect to each other, the rotation appears in its true direction, but if, on the contrary, *L* and *R* are seen in *L'* and *R'*, so in the wrong place with respect to each other, the card will seem immediately to reverse its direction of rotation, and the axis will seem to tilt away from the vertical.

As an observer, viewing the true direction of the rotating card, will generally be unable to distinguish the right place of the two points when he shuts one of his eyes, the circumstances are favourable for the reversing of the direction of the rotating card.

Utrecht, October 5.

L. U. H. C. WERNDLY.

The Interpretation of Mendelian Phenomena.

IF I have read Dr. Archdall Reid's letter in *NATURE* of October 31 aright, he draws a distinction between the study of heredity in general and the study of the problems of sexual reproduction, now defined as the problem of the function of sex (an expression with which I am perfectly contented). Among the problems of sex he includes the study of the actual transmission of characters as dealt with by Mendelians. The novelty of this classification is certainly attractive; but I find it difficult to understand what branches of knowledge remain to fall under the former head. In what does the study of heredity consist if not in the study of the transmission of characters from parents to offspring?

If by this apparent paradox it is only meant to imply that the Mendelians must confine their study to the transmission of characters by the sexual method, they may seek comfort in the reflection that this is by far the most important of all branches of heredity—it is the only one, for example, which affects the human race. Indeed, the rule that all organisms pass through a sexual cycle at some period of their existence has extremely few exceptions; but I, for one, see no reason for restricting the experimental study of heredity even to this extent. Mendel demonstrated the segregation of the germinal representatives of certain characters in the reproductive cells. What reason is there for doubting that such segregation may take place among the ova of a parthenogenetic individual? There is, in fact, evidence of the actual occurrence of such segregation. More than this, we know of segregation where reproduction is purely vegetative, as in the case of bud sports.

It is the claim of the Mendelians that they have dis-

covered in certain cases some of the fundamental characters of an organism—the units of hereditary transmission, which are represented in the reproductive cells by definite entities known as allelomorphs. Sometimes these characters are identical with those which can be defined by simple inspection previous to experimental analysis, sometimes they are not. Sometimes the apparent character depends upon the simultaneous presence of several allelomorphs, each of which may segregate from its opposite in complete independence of all the others. Mendel himself suggested that this conception, the proof of which he left to his successors, might afford the explanation of certain botanical cases which do not appear to be widely different from that of the mulatto. If Dr. Archdall Reid will produce authenticated pedigrees showing the repeated crossing of the mulatto with pure white blood and pure black blood respectively, together with a detailed account of all the offspring produced, he will make a very substantial contribution to our knowledge of heredity in the human race, and one which will be examined with very great interest by Mendelians. In the absence of such evidence the statement that there is no segregation does not seem to me to be justified, even in this particular instance.

It has been pointed out that Mendel's discovery is leading to a change in our conception of the constitution of an organism comparable with the change which the advent of the atomic theory produced in chemists' conceptions of compound substances. Whatever biological problem we may now discuss, Mendel's facts have to be reckoned with. It is true that the only method so far discovered of studying the constituent characters of organisms consists in the crossing together of individuals in which some of the characters are different. This method is so closely comparable with that by which the chemist studies his compounds that Mendelians have often found a readier appreciation of their views among students of the more precise physical sciences than among biologists. The advantage of introducing exact experiment into the study of heredity ought to be obvious to all, and I fail to see any other objection to the method except its novelty.

If Dr. Archdall Reid desires to grasp this new conception, I can only recommend him to a renewed study of the literature of the subject, beginning with Mendel's own papers. Better still, let him repeat a few of the simpler experiments. There is no royal road to this knowledge; but it is knowledge which is rapidly revolutionising our entire conception of the constitution of a living organism. Dr. Archdall Reid is so far from appreciating this at present that further discussion seems likely to be of very little profit. I will conclude my contribution to this controversy with a word of warning. If Dr. Archdall Reid discards Darwin's opinion, based as it was upon an unequalled experience, that domestic and natural varieties have arisen by essentially the same process, he may find himself landed among a crowd of unsuspected difficulties.

Cambridge, November 2.

R. H. LOCK.

Method of Observing the "Subjective Yellow."

A SIMPLE method of obtaining the sensation of yellow produced by the mixture of red and green lights is afforded by a small direct-vision spectroscope of the ordinary kind in which the slit can be rotated to adjust its line perpendicular to the plane of refraction. If the slit is turned slowly from this normal position, the bands of different colours of course take up a sloping direction across the spectrum, like books on a half-filled shelf. As the slope increases, the upper end, for example of the red, closes down on the lower end of the green, and as the two blend the clear yellow tint is produced. Other colour mixtures can be similarly noticed.

It may be added that if the slit is turned thus until its length lies in the plane of refraction, the violet end of the impure spectrum obtained is drawn out and so more easily observed than in the normal method of use, and is still pure enough for most of the purposes for which a simple spectroscope is of use.

JOHN H. SHAXBY.

University College, Cardiff.